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Extract of a Letter from Sig. Secchi to Capt. R.H. Manners, R.N.

"I have lately received the interesting Memoir of M. Otto Struve on the rings of *Saturn*, and I have sought whether the ancient observations of Campani and the early Italian astronomers confirm what has been found by him, that the dark space between the ring and the planet formerly appeared larger than now, and I have found from a figure of Campani that this is really the fact. The figure, of which I send you the original, shows also a faint shadow as a vestige of the inner dark ring which has been lately discovered.

"*Rome, May* 24, 1853."

[The drawing of *Saturn* above referred to by Sig. Secchi, may be seen at the apartments of the Society. It is stated underneath that it was executed by Campani in the year 1664. The maximum breadths of the ring, and of the interval between the ring and the ball, measured roughly with a pair of compasses, appear to be *exactly equal*. The faint shading indicative of the obscure ring is seen *immediately contiguous* to the bright ring at the part where the latter crosses the body of the planet.]

Note on Professor Powell's Communication in the last Number of the Notices, p. 221.

Since the publication of the *Notice* referred to, the author has become acquainted with an apparatus on the same principle as that he has described, though differing in the details of its construction, which is more complex, invented by Bonenberger; a modification of which has been employed in Magnus's researches on some phenomena of rotatory motion in projectiles (see Taylor's *Foreign Scientific Memoirs*, N. S. part iii. p. 223). An apparatus somewhat of the same kind, he is also informed, is in the collection at King's College, London.

June 13, 1853.

Occultation of ο Tauri observed at Ashurst. By Robert Snow, Esq.

(N. Lat. 51° 15′ 58″. W. Long. $1^{m}\ 10^{s}{\cdot}0$.)

	h m s		
Immersion	6 23 30	Corrected sidereal time.	Bright limb
Emersion	7 33 15	— —	Dark limb

Immersion not good. Emersion very good.

Nov. 27, 1852.

On the Determination of the Longitude of the Observatory of Cambridge by means of Galvanic Signals. By G. B. Airy, Esq., Astronomer Royal.

The President resigned the Chair to Dr. Lee, and then gave a *vivâ-voce* account of the operations for determining the Longitude of the Observatory of Cambridge by Galvanic Signals.

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The object for which the galvanic connexion of the Royal Observatory with the London Bridge Railway Station was originally proposed, was the determination of differences of longitude with other observatories, British and Continental. Till within a short time, however, the connexions have been made available, principally, for the transmission of daily signals at certain definite hours along the principal Railways of England: for the dropping of the Signal Ball mounted by the Electric Telegraph Company on the roof of their Office in the Strand: and for the maintenance of the movement of several sympathetic Clocks, one of which is at the London Bridge Station. As soon, however, as the proper turn-plates were mounted at the Royal Observatory, and other details of mechanical arrangement were completed, experimental comparisons were made (comparing the transit-clock at the Royal Observatory with a chronometer at London Bridge, with one at Dover, &c.) in order to learn by experience the most advantageous method of conducting the operation for a real determination of difference of longitude.

The wire from London to Greenwich used in these operations is susceptible of such connexions, that its current may be made to pass through the coils of a telegraph needle which is fixed on the case of the transit-clock, and thus to the earth. This is the connexion which is used when signals are to be received from a distant station. The wire also admits of such connexion that the galvanic circuit, interrupted at one place in the Royal Observatory, passes from the earth through a galvanic battery, then through the coils of the telegraph-needle, then (with an interruption as is mentioned above) to London; and the interruption may be destroyed, or the circuit completed, either (on one arrangement of turn-plates) by an apparatus similar to that of the common speaking-telegraph; or (on another arrangement of turn-plates) by a contact-piece on the eye-end of the transit-circle, which is intended ultimately for use in the American method of recording transits. In either of these cases, if the London or distant end of the wire communicates through the coils of a telegraph-needle to the earth, the completion of the circuit at Greenwich will produce a movement both in the needle at Greenwich and in that of the distant station.

The arrangement of Greenwich turn-plates which, in fact, was used in the observations to be described, was that in which the circuit is completed at the eye-end of the transit-circle. And some of the signals given did actually correspond to the transits of stars over wires of the transit-circle, as observed by the signal-giver, whose eye was applied to the eye-piece, while with his finger he made the contact with the contact-piece.

In the arrangement which has been described, it will be perceived that nothing depends on the accuracy of the signal-giver. It is his business to complete the circuit, and thus to give signals at certain approximate times; but by this operation needle-signals, similar in all essential respects, are given both at Greenwich and at the distant station; and if observers are stationed to observe the motions of the needles, the comparison of the clocks will depend

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on the accuracy of these signal-observers only, who are under the very favourable circumstance of observing signals of similar character at both ends of the line. In order to give to these observations all practicable accuracy, it is desirable that the signal-giver should be at a distance from the signal-observer; and even that, if possible, he should be in another room.

The arrangements made at the distant stations were formed on the same principles, and, in fact, approached very nearly to those which have been described as applying to Greenwich. In one respect, however, they were necessarily less perfect: in no other place is the galvanic-needle carried by the transit-clock; and it was necessary, therefore, at other places to use another clock or chronometer, which was compared by means of transportable chronometers with the transit-clock.

The preceding description shows that at each station it is necessary to have a signal-giver (A) and a signal-observer (B). But it was also found advantageous to employ a third person (C), on account of the following arrangement. The plan adopted as most convenient was, that the signal-giver should fix, in his mind, upon any arbitrary number of signals for each "batch!" and that he should give notice of this number of signals to the signal-observers at both ends by giving the same number of warning-signals at intervals of about 2^s; (3^s is better for a long line.) It was the duty of (C) to watch and count these warning-signals, and then to say "There are [so many] signals coming;" he then remained silent while the signals, at intervals of 10^s to 15^s, were observed by the signal-observer; and when the just number was completed he called out, "The batch is finished." This left the signal-observer (B) free to examine the seconds on the dial, to write down the minutes, and generally to prepare himself for the next batch.

Supposing the signals given in equal numbers from the two ends of the line, the signal-giver (A) at each end was employed during only half the time; but (B) and (C) at each end were employed through the whole time.

The order of operations then, as between Greenwich and Cambridge, was as follows:—

At 11^h p.m. Greenwich Mean Solar Time, Greenwich commenced by giving five signals at intervals of about 2^s each. The turnplates were changed, and Cambridge responded by five similar signals. These were merely to say "All is right." Then Greenwich gave batches of signals as above described, in numbers of from three to nine signals in a batch (some of them being transits of stars), to 11^h 15^m. Then Cambridge gave similar batches to 11^h 30^m. Then Greenwich gave signals to 11^h 45^m, and Cambridge to $12^h{\cdot}0$. This closed the night's signals. From 135 to 150 efficient signals were given; and as the observation of a signal is perhaps rather less accurate than the observation of a transit-wire, the probable error of the mean of these will be fairly comparable with that of the determination of clock-error in an evening's transits.

The evenings selected for the determination of the longitude of Cambridge were those of 1853, May 17 and 18. Mr. Walker,

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Engineer and Superintendent of Telegraphs of the South-Eastern Railway, undertook to make the circuit perfect in every respect from Greenwich to Lothbury; and Mr. Edwin Clark, Engineer of the Electric Telegraph Company, provided carefully for the junctions at Lothbury and the Eastern Counties' station, and for the instrumental and personal aid required at the Cambridge station. To these gentlemen the thanks of Astronomers are due; not simply for their assistance in the present operation, but for the energetic assistance which they have given in the important work of placing the Royal Observatory in galvanic connexion with the rest of England, and making its determinations of time generally useful. Professor Challis arranged the more strictly astronomical part of the operation, in the following manner. On May 17, Mr. Dunkin observed transits and galvanic signals at Greenwich, and Mr. Todd observed at Cambridge. On the morning of the 18th the observers were interchanged, and Mr. Todd observed transits and signals at Greenwich, while Mr. Dunkin observed at Cambridge. The errors of the transit-clocks were determined by two methods: method (A) in which the *Nautical Almanac* stars were employed, the same corrections being applied to the right ascensions of the *Nautical Almanac* for both observatories, but no particular care being taken for the identity of the stars at the two stations; and method (B), in which the clock was corrected by stars in a list suggested by Professor Challis, the same stars being observed at both stations, but no attention being given to the accuracy of the assumed Right Ascensions.

For comparison of the Cambridge transit-clock with the chronometers used at the Railway Station, Professor Challis employed three chronometers. It was found necessary to reject the comparisons of one of these.

It is unnecessary to say that every care was given to the determination of the instrumental errors of the transit-instruments at both stations. At both stations, transits were observed both before and after the galvanic signals, on both evenings. The number of battery-cells used at each station was 72. The air was dry, and the insulation was good.

The results of the operation are as follows:—

East Longitude of Cambridge.

Method (A).

	s
May 17, by 145 signals	22·953
May 18, by 134 signals	22·978
Mean	22·966

Method (B).

	s
May 17, by 145 signals	22·903
May 18, by 134 signals	22·988
Mean	22·946
Mean of the whole	22·956

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This is probably one of the most accurate determinations of difference of longitude hitherto made.

The result arrived at in 1829, by transmission of chronometers, was $23^{s}\cdot 54$.

The President then stated that on May 25 signals were passed (in exactly the same manner) to and from Edinburgh, for the determination of the longitude of Edinburgh Observatory, and, as regarded the galvanic part of the operation, with perfect success. There was, however, something suspicious in the indications of one of the chronometers employed at Edinburgh, which threw doubt on the clock-comparisons, and rendered the apparent determination of difference of longitude doubtful. The following result however was obtained, which is free from the influence of uncertainty in the chronometer-rate; that when a signal is given at Greenwich, by means of a Greenwich battery, the time noted for the signal at Edinburgh is later than that noted at Greenwich by $\frac{1}{17}$th of a second of time (and *vice versâ* if the signal is given at Edinburgh by means of an Edinburgh battery). This difference probably arises from two causes: first, the time actually occupied by the transmission of the galvanic pulse, which, according to the American determinations, would explain less than half of the difference; secondly, the circumstance that the galvanic current when it reaches the distant needle is somewhat less vigorous than when it passes the nearer needle, and the languid movement of the distant needle catches the eye more slowly, and is recorded as occurring at a later time.

In reply to a remark by Mr. R. C. Carrington, on the influence of the length of the transit telescope upon the determination of clock error, the President stated that the length of the Greenwich transit telescope is less than 12 feet, and that at Cambridge 10 feet, and that no sensible error would result from that cause.

On Certain Results of Laplace's Formulæ, expressing the Relation between the Inclinations, Excentricities, and other Elements of the Planetary Orbits. By Dr. Lardner.

In this paper the author shows that the well-known formulæ of Laplace,

$$\Sigma\,(m\,\sqrt{a}\,e^2) = c \qquad \text{(I)}$$

$$\Sigma\,(m\,\sqrt{a}\,\tan^2 i) = c' \qquad \text{(II)}$$

would not of themselves suffice to assure the stability of the planetary system in the sense in which the term is usually applied. By a very simple process he demonstrates that for anything contained in these theorems to the contrary, the excentricities of all the planets which revolve within the orbit of *Jupiter* may attain any magnitude whatever, that their several orbits may actually become